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FINANCIAL INCENTIVE EFFECTS AND INDIVIDUAL DECISIONMAKING

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ABSTRACT

Though much of the literature of experimental psychology covers topics that seem relevant to economics, the literature is generally ignored by economists. Possibly the reason for this is that psychologists seldom use financial incentives to motivate subjects' choices. This paper provides an example of an individual decisionmaking experiment in which the presence or absence of financial incentives affects the subjects' behavior. The observed effects are not marginal but often involve qualitatively different types of responses.

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Economists have become interested in a number of theoretical and policy related areas in which the results depend on fairly specific details of individual or group decisionmaking processes. Consider, for example, structure of markets with imperfect information, e.g., labor markets (Mortenson 1976, Wilde 1977) and consumer product markets (Salop and Stiglitz 1977, and Wilde and Schwartz 1979). In this literature it appears that properties of market equilibria depend upon the search strategies of economic agents (cf. Salop and Stiglitz 1977, and Wilde and Schwartz 1979). The effects and desirability of intervention in such markets, e.g., truth in lending, labeling type laws, or provision of insurance against natural disasters, etc. depends upon individuals' decision strategies (cf. Slovic et al. 1977, and Schwartz and Wilde 1979). Many important economic decisions are made by groups in nonmarket settings, probably the most obvious examples being legislative bodies and courts.

Experimental laboratory methods have been successfully used for the evaluation of theories of group decisionmaking. It is well established (Fiorina and Plott 1978, and Plott and Smith 1978) that the lack of proper incentives can lead to qualitatively different outcome choices by

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committees. Thus, when Grether, Isaac, and Plott (1979, 1981) studied the committee procedures used to allocate landing rights at the nation's high density airports, all experiments involved substantial monetary rewards for successful subjects.

A substantial body of experimental research on individual decisionmaking under uncertainty exists, most of it the work of psychologists (see Grether 1978, Slovic, Fischhoff, and Lichtenstein 1977, Hogarth 1975, Slovic and Lichtenstein 1971). It seems safe to say that this work is, to a considerable extent, ignored by economists. While it is beyond the scope of this paper to explain completely why this is so, it is obvious that an important reason is that experimental psychologists generally (though not always) do not use financial incentives to induce "accurate" judgments by the subjects in the experiments. In fact, it is sometimes argued that laboratory data generated under proper incentives are no more useful than the results based on hypothetical choices (Kahneman and Tversky 1979). The failure of incentive effects to show up in decisionmaking experiments (Grether 1980, Grether and Plott 1979) is also noted (Kahneman and Tversky 1979, and Tversky and Kahneman 1981) as evidence that monetary incentives are not needed.

One cannot claim that financial incentives are never used by psychologists, or that when used, they make little difference if any. Siegel (1961) reported that increasing the financial incentives leads to behavior closer to the optimal strategy in probability learning experiments. Also, Slovic (1969), Edwards (1953), and MacCrimmon, Stanbury, and Wehrung (1980) demonstrate that individual choice

behavior and apparent attitude towards risk are sensitive to the use of real money as opposed to hypothetical gambles. See also Slovic, Lichtenstein, and Edwards (1965). In some studies, e.g., Lichtenstein and Slovic (1973) and Goodman, Saltzman, and Edwards (1979) financial incentives are employed, but these studies represent exceptions to normal practice. Even when such incentives are employed, there may be problems about the precise ways the incentives are structured (for elaboration of this point see Grether 1978, Grether and Plott 1979).

The purpose of this paper is to present evidence that, for types of decisions that are frequently considered by psychologists and are relevant to economists, modest financial incentives can affect individual performance in ways that can have substantial impact upon the evaluation of theories concerning individual behavior. That is, the behavior of financially unmotivated subjects may be qualitatively different from that of subjects whose rewards are dependent upon their performance.

The experiment described in the following section was designed to test the representativeness heuristic. For a discussion of several decisionmaking heuristics, see Kahneman and Tversky (1972, 1973, 1979); and Tversky and Kahneman (1971, 1973, 1974, 1981); and for the results of the experiment, see Grether (1980).

EXPERIMENTAL METHODS

The experiment was conducted using as subjects students from University of California at Los Angeles. Students were recruited from economics classes. They were told that an "economics experiment" was to be held, given the time and place of the experiment, told that the experiment would not last longer than one hour, and that the minimum payment would be five dollars. When subjects arrived, they were randomly sent to one of two different rooms. Procedures in the two rooms were identical except for method of payment. A total of ninety-nine people participated in the experiment.

Instructions were passed out and after obtaining the participants' names, social security numbers, and addresses, the first three paragraphs of the instructions were read out loud to the subjects (see Appendix).

At this time the subjects elected one of themselves to be a monitor. The monitor was allowed to inspect all equipment, and, more importantly, to observe all procedures during this experiment. It was stated in the instructions that the monitor "should check the truthfulness of what the experimenter says, but other than that may not communicate any information to you in any way. If the monitor communicates any other information, he or she will be asked to leave without payment."

Though subjects could watch the experiment being prepared, it was hoped that the use of the monitor would further increase credibility. After the monitor was elected, the remainder of the instructions were read and subjects' questions answered. Once it

appeared that subjects understood what their tasks were, the procedures were gone through (i.e., a dry run so to speak) to make sure that the subjects fully understood the mechanics of the experiment.

The equipment consisted of four bingo cages and an opaque screen. One of the cages designated Cage X contained balls numbered 1 through 6, and another cage, Cage Y, contained balls numbered 1 through 50. The other two cages (Cage A and Cage B) each contained six balls marked with the letters "N" or "G" (four N's and two G's in Cage A and three N's and three G's in Cage B).

The experiment proceeded as follows: Cages A and B were put behind the screen and the rule used to determine which of them was to be chosen was announced; Cage X served as the "prior." The rules were of the form "if one of the numbers one through k is drawn from Cage X, we shall choose Cage A; otherwise we shall choose Cage B." k was varied between two and four, thereby generating prior odds ratios for Cage A of one-half, one, and two. Next, a ball was drawn from Cage X (the subjects could not see the number on the ball) and, depending upon that number, either Cage A or Cage B was chosen and a sample of size six was drawn (with replacement) from it. After each draw the result ("N" or "G") was announced, written on a blackboard, and the subjects also recorded it on forms provided. After the six draws were completed, subjects were asked to indicate on their forms "the one (cage) you think the balls came from."

Next subjects were given the opportunity to choose to play one of two "bets" called the "Cage Bet" and the "Number Bet." A subject won the Cage Bet if the cage the subject picked (A or B) was indeed

the cage the balls were drawn from. For the number bet the experimenter draws two balls (with replacement) from Cage Y, and if the number on the second ball is less than or equal to the number on the first ball, the subject wins. Which bet the subject played was determined as follows: subjects circled a number from 0 to 50 on the form provided. If the number circled was less than the first number drawn from Cage Y, the subject played the number bet; otherwise the subject played the Cage Bet.

Instructions (see Appendix) contained two examples which illustrated the procedures described. Other than these examples and the practice run-through, subjects received no training, nor did they receive any further instructions or feedback during the course of the experiment. After subjects had chosen the cage and number, the procedures were repeated using possibly a different rule for the prior.

Notice that the procedures are a variant of the method of Gordon Becker, DeGroot, and Marschak (1964) for determining demand prices. Thus it is a dominant strategy for the subjects to reveal their true personal probabilities. In other words, there is no incentive for the subjects to lie or otherwise distort their reported probabilities. For example, if a subject's subjective probability for Cage A is between .6 and .62, the subject should indicate that Cage A is more likely and circle the number 30. If a subject's subjective probability is exactly .6, the subject should be indifferent between circling 30 and circling 29.

Suppose that a subject's subjective probability is .59, but that the subject circles a number other than 29. If the number circled

is less than 29, then the subject runs the risk of playing the number bet with a probability of winning that is less than .59. Similarly, if a subject circles a number greater than 29, say 38, then if numbers 30 through 38 are drawn, the subject will play the cage bet and miss the chance to play the number bet at odds more favorable than .59/.41.

Table 1 gives the posterior probabilities for all possible outcomes used and the "correct" number to circle, i.e., that based upon the objective probabilities for each outcome.

In one room all subjects were paid \$7 for participating.

In the other room subjects were told that at the end of the experiments one of their decisions would be randomly selected (using a bingo cage) and that those whose decisions were correct, i.e., those that won the relevant bet (Cage Bet or Number Bet) would receive \$15, otherwise \$5.

RESULTS

While preparing the data on subjects' responses, the circled numbers were converted into estimates of the subjects' personal probabilities. Note that given the procedures subjects should never respond by circling numbers 0 through 23 and only those subjects whose personal odds on the cage bet are exactly one to one could reasonably respond by circling 24. This assumes, of course, that subjects would rather win their bets than lose them.

Some subjects did in fact give such responses and, as can be seen from Table 2, the frequency of occurrences was greater for those without financial incentives than for those with financial incentives. The difference is statistically significant at any

TABLE 1

A. POSTERIOR PROBABILITIES OF CAGE A

Prior Probability for A	Number of N's						
	0	1	2	3	4	5	6
2/3	0.149	0.260	0.413	0.584	0.737	0.849	0.918
1/2	0.081	0.149	0.260	0.413	0.584	0.737	0.849
1/3	0.042	0.081	0.149	0.260	0.413	0.584	0.737

B. POSTERIOR ODDS FOR MOST LIKELY ALTERNATIVES

Prior Probability for A	Number of N's						
	0	1	2	3	4	5	6
2/3	5.70:1	2.85:1	1.42:1	1.40:1	2.81:1	5.62:1	11.24:1
1/2	11.39:1	5.70:1	2.85:1	1.42:1	1.40:1	2.81:1	5.62:1
1/3	22.78:1	11.39:1	5.70:1	2.85:1	1.42:1	1.40:1	2.81:1

C. OBJECTIVE OPTIMAL RESPONSE

Prior Probability for A	Number of N's						
	0	1	2	3	4	5	6
2/3	42	37	29	29	36	42	45
1/2	45	42	37	29	29	36	42
1/3	47	45	42	37	29	29	36

TABLE 2
RESPONSES FOR SUBJECTIVE PROBABILITIES

A. NUMBER OF RESPONSES			
	Clearly in Error	Others	Total
With Financial Incentives	42	918	960
Without Financial Incentives	126	854	980
Total	168	1772	1940

B. NUMBER OF PEOPLE MAKING ERRORS			
	Some Errors	No Errors	Total
With Financial Incentives	15	33	48
Without Financial Incentives	32	17	49
Total	47	50	97

conventional level of significance ($\chi^2(1) = 44.1$). Figures 1 and 2 give the frequency distributions of errors for the two groups. Notice that the greater number of mistakes by those not under financial incentives for accuracy was not due to just a few people making a large number of errors; in fact, nearly twice as many subjects made errors in the incentive group as in the group without financial incentives. This difference is also highly significant statistically ($\chi^2(1) = 11.3$). In addition to those responses which are best interpreted as errors, some people always (or nearly always) indicated that these subjective probabilities were unity. Some people began indicating probabilities different from one and after a few trials switched and circled 50 for the rest of the experiment. There were five subjects with financial incentives and nine subjects without who circled 50 nearly all of the time. The result is not statistically significant ($\chi^2(1) = 1.2$) though it agrees qualitatively with the other counts.

Thus, if one takes the number of people making "nonsense" responses or apparently not responding to the different sets of data, the overall result is that the rate of such occurrences is much higher and statistically significantly so for the group without financial incentives. People often make mistakes, so possibly one should not be concerned about a single or a very small number of evident errors. Those who make several obvious errors are likely to be confused, so all their responses are suspect. Thus the total number of questionable responses equals the sum of the number of responses of confused subjects, the answers of the nonresponsive subjects, and the mistakes

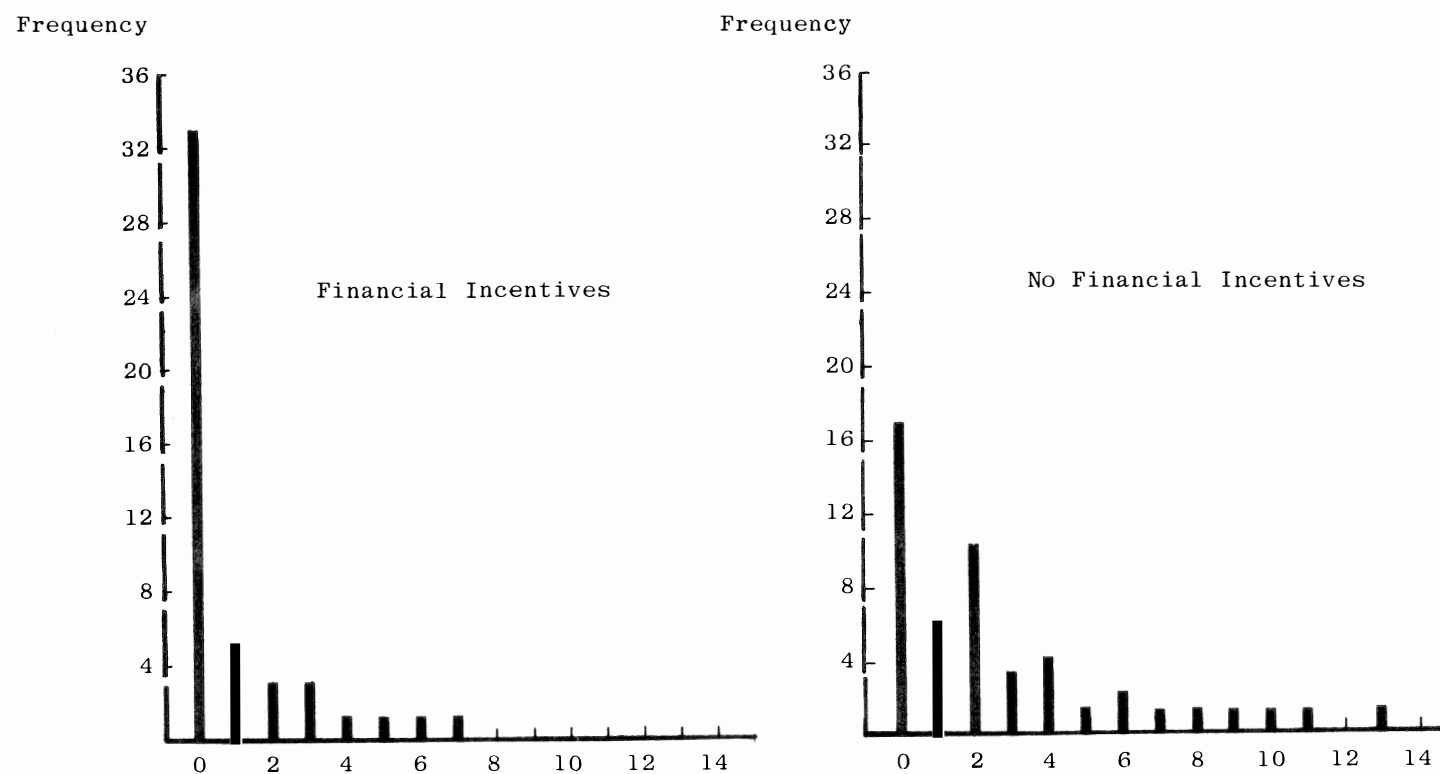


FIGURE 1
Frequency Functions
Number of Errors by a Single Subject

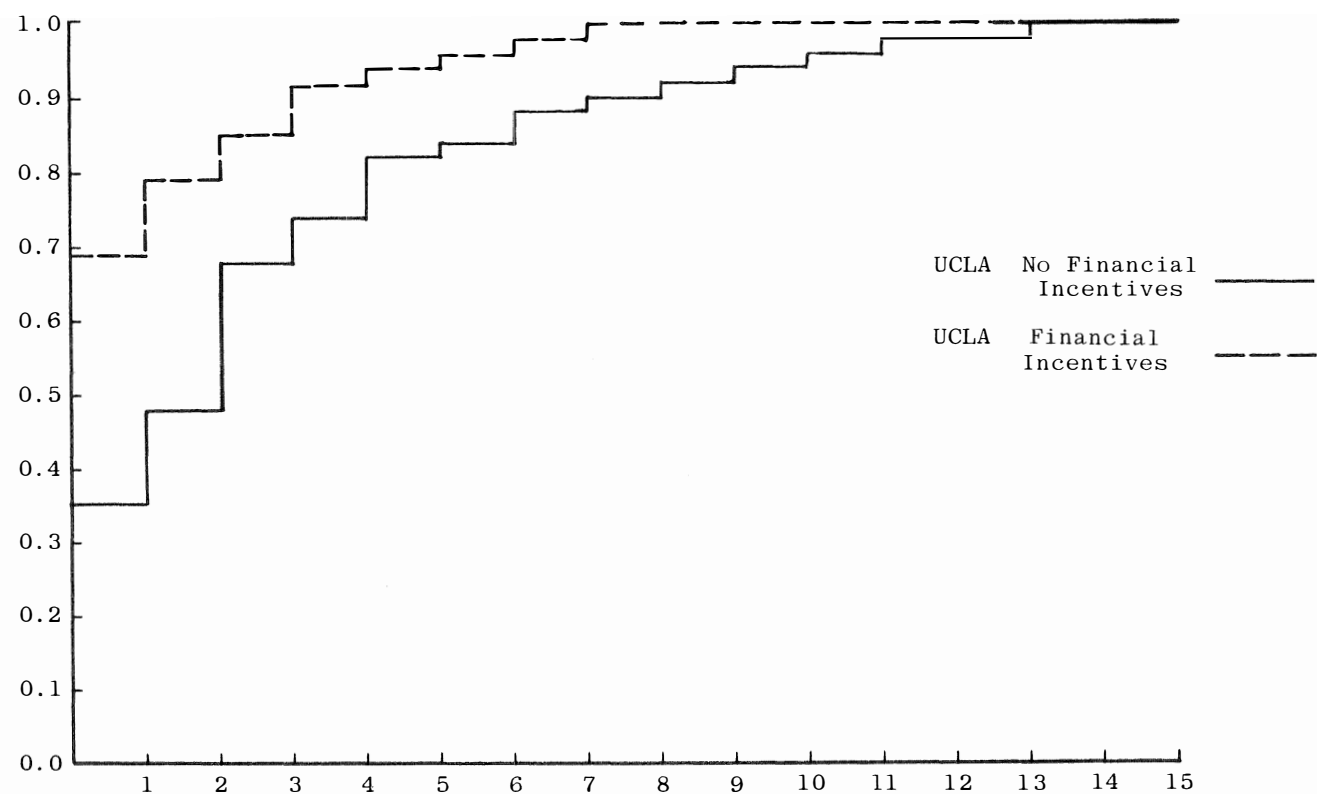


FIGURE 2
Sample Distribution Functions
Number of Obvious Mistakes by a Single Individual

made by other subjects. Where one draws the line between occasional and persistent errors is to some extent arbitrary; for example, one convention could be that those who make five or more clear errors (out of twenty responses) are confused overall. Table 3 gives the number of responses dropped using this criterion. The effect of financial incentives is as expected and statistically significant ($\chi^2(1) = 126.6$). Table 3 is only exemplary. Using the data in Table 2 and Figures 1 and 2, readers can use their own criteria for confused subjects. If one uses individuals dropped rather than responses, this naturally decreases the chi-square as each person represents roughly twenty responses. Applying this rule for the data underlying Table 3 gives $\chi^2(1) = 5.0$ which is still statistically significant.

It should be noted that "blanks" or nonresponses were extremely rare under both conditions. This could possibly be because subjects felt that they might not receive payment for participation if these answer forms were not completely filled out. Also, the results suggest that restricting the possible responses to the interval from one half to one, as is sometimes done (Lichtenstein and Fischhoff 1977) may affect the apparent quality of the responses. Detailed statistical analysis of the responses of the subjects is beyond the scope of this paper and will be presented elsewhere.

TABLE 3
RESPONSES DROPPED DUE TO ERROR, CONFUSION, OR NONRESPONSIVENESS

	Dropped	Not Dropped	Total
With Financial Incentives	167	793	960
Without Financial Incentives	398	582	980
Total	565	1375	1940

SUMMARY AND CONCLUSIONS

In this paper some evidence of the effects of financial incentives in decisionmaking experiments has been presented. Note that the behavioral effects noted are not of the "fine tuning" type (i.e., is the mean response for Group A significantly different from that of Group B?), but refer to gross aspects of behavior, namely, the rates of occurrence of clearly confused or strange responses and of nonresponsiveness.

The behavior differences observed here are not merely increased variability that could be compensated for by taking larger samples. Rather, the behavior observed is qualitatively different. This finding is consistent with the findings of Fiorina and Plott (1978) in their study of majority rule committees. It appears that proper incentives are useful not merely to make economists pay attention to the experimental results, but also to insure cooperation and attentiveness on the part of the individual subjects. Thus, for those questions of public policy and for problems of theory testing for which experimental methods are necessary or appropriate, the use of proper financial incentives can be of critical importance.

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